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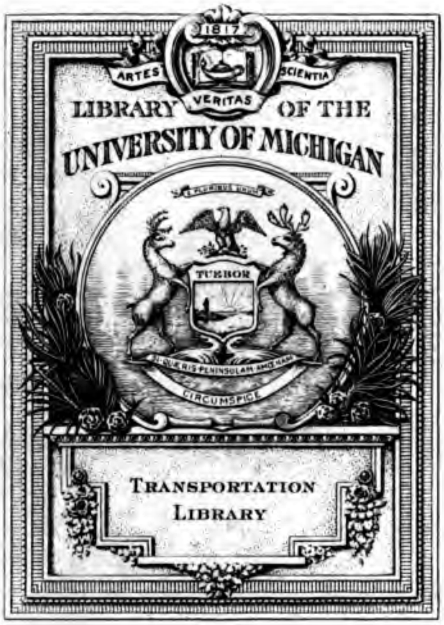
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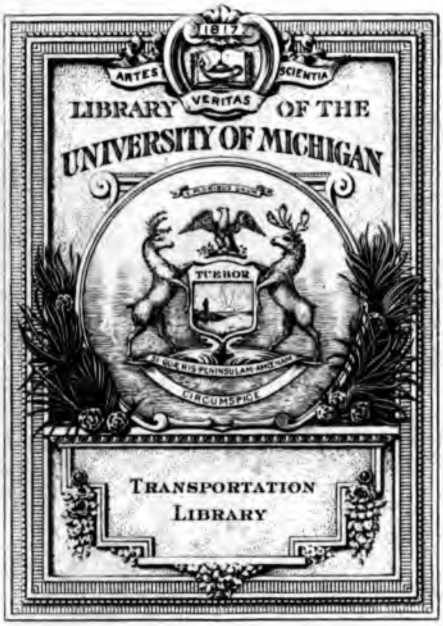

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THE

PECULIAR FEATURES

OF THE

ATMOSPHERIC RAILWAY SYSTEM.

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GEORGE BERKELEY, Assoc. Inst. C. E.

WITH AN ABSTRACT OF THE DISCUSSION UPON THE PAPER.

EXCERPT MINUTES OF PROCEEDINGS

OF THE

INSTITUTION OF CIVIL ENGINEERS.

LONDON.

1845.

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INSTITUTION OF CIVIL ENGINEERS.

April 15, 1845.

Sir JOHN RENNIE, President, in the Chair.

No. 716. "The peculiar features of the Atmospheric Railway System." By George Berkeley, Assoc. Inst. C.E.*

When it is considered expedient to appoint a Committee of the House of Commons, to report upon the merits of the atmospheric system, it is fit that such facts relating to it, as have been clearly ascertained, should be presented to, and be discussed by the leading Members of the Institution, in order that the body of the profession may form a correct estimate of its real importance; and that the discovery and the development of truth may be promoted, by the influence of a more generally prevailing opinion amongst them.

The introduction of so important and comprehensive a subject, by so young a member of the profession, may be thought to require an apology, but the author having been connected with the early progress of the invention, under the late lamented Mr. Jacob Samuda, (Assoc. Inst. C. E.,) and having since, in conjunction with Mr. Marshall, been entrusted by Mr. Robert Stephenson, with the responsibility of carrying out his instructions, in trying the series of experiments for his Report, will it is hoped acquit him of presumption.

It is proposed in this paper, to submit to the consideration of the Members of this Institution, questions and facts which are generally received, and whilst abstaining from giving any personal opinion, to induce a discussion among the senior members of the profession.

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* The discussions upon this paper extended over three evenings, but an abstract of the whole is given consecutively for the sake of facility of reference.

Before entering directly upon this subject, it is necessary to free it from the influence, which has been used to insure the belief of its great advantages, and its general adoption, by the fact of two eminent engineers being connected with it.

It may be asked whether these gentlemen are pledged to support its general adoption.

It is certain, that by one (if not by both of them), its application has only been sanctioned to lines of railway, presenting peculiar features, and at the same time, it is recommended, that the gradients, weight of rails, &c., should be such, that if from any cause, it should be necessary to revert to the locomotive system, facilities should be presented for doing so.

If they have so completely decided in its favour, why should they so partially adopt it, and sacrifice some of those which are termed its chief advantages?

The mechanical difficulties in the application of the atmospheric system, are perhaps the least important for consideration, as it may be assumed, that the experience and ingenuity of mechanics will overcome them; but as the late discussion* was terminated, before these were fully discussed, they should be briefly noticed, in order to elicit further remarks on the subject, as well as some description of the means now being adopted for overcoming these difficulties.

Some uncertainty and difference of opinion appeared to exist, with regard to the facilities of working the traffic, both in terminal and intermediate stations, and the necessary arrangements at level crossings.

At terminal stations, where the goods or mineral traffic is large, a locomotive engine is now required to do the work, but it is also usefully employed as a "pilot" engine on the main line.

Some kind of power will still be necessary at such stations, but if different from the power employed on the main line, it cannot be there used with equal convenience or economy.

At intermediate stations, the waggons are now "shunted" to their proper places, in a siding, by the engine which has propelled them along the main line.

Can this be effected by the atmospheric system? If not, must not additional power be here employed, either in the form of manual labour, horse, or engine power? and this, in addition to the risk of accident, from leaving waggons in the main line?

One of the patentees stated in the late discussion, that at level crossings, by means of a cylinder and piston, a barrier would be raised;* from this it may be presumed, that the cylinder and piston and barrier are connected by mechanism.

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* Vide Minutes of Proceedings, 1845, p. 143.

It has been proved by experience, that little inconvenience, and no danger, exists at level crossings, when a man is employed, and he has only to open and shut a gate; but it is doubtful whether this would be equally true, when, between his intention and the effect, there is the efficient working of a train of mechanism, which probably he does not understand.

Again, this complex arrangement, to effect what is usually done by the most simple means, will be costly.

In many instances, level crossings have been avoided by some of those who have sanctioned the system, at the cost of a bridge with approaches, which is a great addition to the expense of the construction of the line.

Some other-important points naturally suggest themselves, which, as they are cited by some, as advantages belonging to the new system, and are not admitted, as such, by others, may be beneficially discussed.

Running trains much more frequently, has been considered essential to secure the advantages of the atmospheric system.

On long lines of railway, the distance each person travels, is as important as the number of travellers.

There is always some uncertainty, as to the number that will be found at the various stations.

Each train must be equal to convey all.

Will not therefore the gross weight be increased in a greater proportion than the nett weight of passengers? From this a loss of power must arise.

Would the running of very frequent trains, on long lines, be a convenience equivalent to the loss and increased expense consequent upon it?

The case is different on short lines, where the number of passengers is large, and the character of the traffic simple.

It must be granted, that the increased number of trains would be, and are justified, and that great convenience results from them.

It has been stated by the advocates of the atmospheric system, and has been admitted by some who do not adopt it, that a greater speed has been attained by its adoption, but with the attendant contingency of greater cost.

The greatest speed observed during the series of experiments, published in Mr. Robert Stephenson's Report, was between 36 miles and 40 miles per hour, with a train of 26·5 tons. It is known that locomotives, with trains of double this weight (though on better gradients), have attained velocities varying from 50 miles to 55 miles per hour.

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* Vide Minutes of Proceedings, 1845, p. 143.

The velocity that would be theoretically attained, with the apparatus at Dalkey, supposing no leakage or other loss to interfere, would be little more than the highest of these, or about 60 miles per hour.

Under these circumstances, it is clear, that to maintain higher velocities on the atmospheric, than with the locomotive system, more power must be employed than is now used at Dalkey, or about 200 H. P. for 1¼ mile.

The leakage, which influences the velocity of the train, will be greatly increased, by extending the length of the pipe to 3 miles or 4 miles, as proposed, and this will also augment the amount of power, that must be employed, before the advantage of greater speed can be realized.

Does this justify us in expecting, that greater velocities will be maintained by the atmospheric, than by the locomotive system, or are those who differ from this opinion, correct in stating, that it must be attended with an expense so inordinate, " that it is just as bad as being mechanically impossible?"

To obviate the enormous first cost of a double line of atmospheric apparatus, it is proposed and stated, as an advantage peculiar to the system, that a single line of rails may be adopted, and that such would prove sufficient.

The impracticability of this is clearly shown in Mr. Robert Stephenson's report, when applied to a line of 112 miles in length, with trains every half-hour, and a mean velocity of 30 miles per hour is maintained, by the fact, that " the total time of the journey would be increased to 10 hours," in consequence of the delays, occasioned by one train waiting, till it had met that which was coming in the opposite direction, and the pipe was exhausted so that it could proceed.

On shorter lines, perhaps these obstructions would not be so objectionable; but it would be as easy for locomotives to follow each other, with trains every half or quarter of an hour, and when arrived at a station, to wait till the train in the opposite direction had passed.

The only reply to this, is in the words of Mr. Brunel, who says, " I have no doubt that the atmospheric, upon a single line, is considerably safer than a locomotive on a single line, which I do not think safe."

With regard to the safety of single lines worked by locomotive power, the Yarmouth and Norwich Railway, which is a single line of 20 miles in length, has worked safely for nearly 12 months. The Northampton and Peterborough Railway also, which is a single line of nearly 50 miles in length, has been sanctioned by Committees of the legislature, under the strongest opposition of land-owners perhaps ever known.

There is no reason why the latter system should not be as safe as the former; the same agent (man) is employed in both cases. By the agency of the electric telegraph, an instantaneous and unerring communication is carried on.

Can it be supposed that a man who is responsible for the act, will start an engine to come in collision with another, which he has but a few seconds before, acknowledged is coming in the opposite direction and on the same line? or, will the engineman risk his own life by disobeying his orders? The experience, of the working of nearly 90 miles of single line, on the Eastern Counties Railway, has shown this to be scarcely possible, under ordinary circumstances.

If, therefore, the experience obtained of the working by locomotives of single lines, has given no cause to call them in any way unsafe, and if reason demonstrates, that they present almost perfect security, what is the foundation for this opinion of their insecurity?

With respect to the additional safety, arising from tieing down the carriage, in the centre of the line. The question suggests itself, will the carriage be tied down, when passing over points and crossings, where there is almost the only liability of running off? or if it be so, is it an advantage?

Is not the chance of collision at sidings, which are worked only by momentum, greater, than where there is the control of a locomotive engine? and are not these the places where the greatest, if not the only liability of such collisions, exist?

It is said, that the comfort of passengers will be enhanced, by the adoption of the atmospheric system; that they will not be annoyed by the dust and noise, which they now experience. That small portion only which is due to the locomotive engine will be got rid of, and how small that portion is, can be well estimated, on any line which traverses a sandy district.

The power of stopping the train is considered by some "greater on an atmospheric than on a locomotive line." Does not the power of stopping, as well as the momentum increase with the gravity?

By removing the engine and tender, the weight which is the most concentrated and therefore the most economically and efficiently applied, is removed. In the case of the atmospheric railway, the moving power as well as the momentum must be overcome by the gravity of the train, which will be divided into many parts, and must therefore be applied by many hands, while with the locomotive, the moving power, as well as the more concentrated effect of gravity, will have to overcome only the momentum.

If, with the atmospheric system, a greater number of hands must be employed, to produce the same proportionate effect of adhesion, to overcome the momentum, where are the advantages in this respect?

Having thus briefly stated some of the principal points mooted in the working of railways, it is necessary to introduce the first cost of construction and the expense and facilities of maintenance.

The discussion of this part of the subject must be interesting, not only as it involves the most important subjects for consideration, but also, because those engineers who have let contracts for its application, to lines of railway, on which they are professionally engaged, will be able to afford information respecting its first cost; and others, who have had experience and are well informed of the cost and facilities of maintaining locomotive lines, can give opinions, which may be depended on, concerning the comparative advantages which will be afforded in this respect, by the application of the atmospheric system.

In a country where surface lines can be made, it appears to be agreed, that the application of the atmospheric apparatus occasions a considerable additional outlay; for instance, in the case of the Yarmouth and Norwich railway, the interest of the necessary additional outlay, would be nearly equal to the whole working expenses of the line, and would very considerably exceed the cost of locomotive power.

Before considering the first cost, when the system is applied in a country of a rougher character, it is essential to determine, what advantages it presents in overcoming worse gradients, than exist on locomotive lines; as it has been stated, that the saving by diminishing the works, (in consequence of good gradients not being required,) is equivalent to the extra cost of the apparatus.

Where bad gradients exist, on locomotive lines, the gravity of the engine and the train, due to the incline, has to be overcome, in addition to the friction and the resistance of the atmosphere.

The power of the engine is applied, as tractive force, to the train, by means of the adhesion of the periphery of the wheels to the rails, and when the gravity, due to the angle of the incline, is very great, the adhesion is not sufficient to overcome it; but as the present weight of the engines is great, and is all applied to produce adhesion, it is fully equal to convey the whole power of the engine; consequently it is the power of the engine that limits the gradient which can be surmounted.

To overcome the resistance, therefore, another engine must be added, or, as the resistances are increased, by gravity, the speed must be diminished.

From these and other causes, the inconvenience and expense of working bad gradients, on locomotive lines, is very great, and their application limited (at all events) to that point, where the friction and gravity of the engine alone, cannot be overcome by its power.

On bad gradients, on the atmospheric system, it is evident, that although every ton weight of the train, will offer an equal resistance (due to the angle of the incline) as with the locomotive system, the weight of the engine itself, will not have to be overcome, as the power is stationary; but, is there no loss with the atmospheric system, which is equivalent to this?

On bad gradients, is not a higher vacuum required?

The effect of a given amount of leakage is increased in proportion as it is rarefied in the tube. The resistance to the air-pump piston increases up to 22 inches, which is the highest that could be calculated upon in practice, and consequently even for the same bulk, to be pumped out of the pipe, more power must be expended. Are not these losses occasioned by the greater rarefaction of the same bulk of air, leaked into the pipe at the density of the atmosphere, and, consequently, the increased bulk to be pumped out, and the greater power required to overcome the resistance to the air-pump piston, due, (up to 22 inches,) to the higher vacuum, fully equivalent to the loss by the gravity of the locomotive engine?

The effect of increasing the inclination of the road, or the weight of the train, on the same gradient, are identical, both simply requiring greater power to overcome the increased resistance.

The losses shown in Mr. Robert Stephenson's Report, as existing with trains of 30 tons and 59 tons, are 57 per cent. and 74 per cent. respectively, or an increased proportion of loss of 17 per cent., due to doubling the weight of the train, which would be about equivalent to ascending a gradient of 1 in 150, at the mean velocity, if all the resistances be considered; but by the common method, of taking only 10 lbs. to the ton, it would be only equal to a gradient of 1 in 230.

Would the loss due to the gravity of the engine on these inclines, be greater than this?

If it be stated, that bad gradients may be overcome, without this loss, by increasing the size of the pipe, and consequently augmenting the power; complexity must ensue. The first cost, and the costs of working are thus augmented and the chief objection to the system is rendered greater. Will these not be equal to the loss by the gravity of the engine?

If from the discussion of this point, it is agreed, that the loss by the introduction of bad gradients is as great on the one, as on the other system, the application of the atmospheric system, is confined to the same narrow limits, that other stationary systems occupy, and it must rank with these, only as a means of overcoming lengths of such bad gradient, as do not come within the limits of locomotive power; or where the lines are short and the traffic great, terminal and simple. The question of its application, to a comparatively rough country, does not differ, except in the proportion, which the cost of the apparatus is, of the total first cost, from the former question, when surface lines can be obtained.

The advantage of economy from the use of lighter rails, would be at once admitted by those who have not much studied the subject, had not those eminent engineers, who have sanctioned its application, sacrificed it, and had we not the highest authority for holding the contrary opinion.

Under these circumstances of additional first cost, is it advisable, that the atmospheric system should be adopted, unless some great increase in the convenience, and saving in the cost of working, can be calculated on?

To this part of the subject it is necessary to direct the attention of those who have had the greatest experience, and who best know the expenses of the working and maintenance of railways.

With respect to the cost of working on the atmospheric system, we need only refer to the estimate given in Mr. Robert Stephenson's Report, where it is compared with the actual cost on the London and Birmingham Railway, and amounts to £73,990 per annum, while the cost is £63,834 per annum.

This does not include maintenance of ways, but is strictly confined to the cost of power.

In this estimate, however, are not some items of expense evidently omitted?

When Mr. Stephenson's experiments were tried, at Dalkey, and when other published experiments, respecting the loss by leakage, were taken, three men were employed on a length of 1½ mile, to supply the composition when it broke away, and to keep it in a soft and efficient state.

The whole of the experiments had the advantage of this special attendance, but no notice is taken of it when the expense is estimated, although the cost of the labour would be £100 per mile.

But supposing this to be a part of the maintenance of way, will not the total expenses of locomotive power amount to little more, or even less, than the mere interest of the additional first cost, by the application of the atmospheric apparatus, to a single line on most of the railways of England.

The notice of one or two remarks of eminent engineers relative to the cost of working may not be considered inappropriate.

Mr. I. K. Brunel says, "That by the use of large boilers, fixed on the most approved plan, by husbanding the power, working the steam expansively, and using the present known improvements in stationary engines, the system would prove as economical as it was free from danger." *

Are we to suppose that, in the application of this ingenious system at Dalkey, the known improvements in stationary engines were not adopted in an experiment, on which it must have been considered the success of the invention greatly depended?

Concerning Mr. Gregory's estimated loss at high speeds "in the cylinders alone, amounting to about 50 per cent. of the pressure," and this result, "in addition to the acknowledged mechanical defects of locomotive engines, at high velocities," while, when "running at slow speed, nearly the whole pressure of the steam had been effective in the cylinders," † we must ask, if, because the steam has not entered the cylinders and is consequently not there indicated, at the same pressure, at a high as at a low velocity, is it of necessity lost? Or, if the idea, that this difference in the loss in the cylinders, at high and at low velocities, be not intended to be conveyed, is not the loss, which takes place in the cylinders, the most important of the acknowledged defects, at any velocity, and therefore should it be stated as a new fact, proved by recent experiments, showing a loss of great magnitude, in addition to those acknowledged?

The atmospheric system is said to present considerable economy in the maintenance of way.

The question involves two considerations :—

1st. The amount of work to be done, and—

2nd. The facilities afforded for doing it.

The cost of keeping the permanent way of a double locomotive line in repair does not exceed £80 or £100 per mile per annum, exclusive of slips.

In the one case, the heavy locomotive engine, the train, and the frost and wet weather, will all occasion the necessity of repair.

With the atmospheric system, only the two latter will produce any effect; but to this must be added, the attention which will be required for the pipe.

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* Vide Minutes of Proceedings, 1845, p. 147.

† Ibid., p. 146.

These will exist under ordinary circumstances.

Which of them presents an advantage over the other?

The case is much influenced, however, when the comparative amount of work, which will result from slips, or any accident to the upper works, is considered.

As conclusive evidence on the advantages of the atmospheric system, in respect of the cost of maintenance of way, Mr. Samuda says "The Dalkey line was quite new, and had worked through the winter, without any stoppage, either from subsidence or slips, and the company found no more difficulty, in keeping that part of their line in order, than they did with the part worked by locomotive engines." *

The Dalkey line is, for the most part, a cutting in sound rock, and has the appearance of a large uncovered stone drain; while the line worked by locomotive engines, is, for a great part of its length, a sea embankment, in some places dividing the waters of Dublin Bay, over which the waves dash with such violence as occasionally, for a short time, to stop the traffic of the line.

Under these peculiarly favourable circumstances of the Dalkey line, with respect to the cost of maintenance, can this be taken as a fair precedent, or be justly compared with the Dublin and Kingstown Railway?

The facilities afforded, must not be considered less important, than the amount of work to be done, for on this head Mr. Robert Stephenson says, in his evidence on the Croydon and Epsom Railway Bill, "That the want of facility, in repairing an accident, will alone prove an insuperable difficulty."

Will not great inconvenience, if not insurmountable difficulties exist, on the atmospheric system, in the mere ordinary repair of the road and of slips?

How are the contractor's waggons to be conveyed, with the necessary supply of ballast and other materials?

How are they to be collected to one spot, and how will the contractor be enabled, to convey and tip, the great quantity of gravel, &c., which is often essential, to prevent the remaining portion of the embankment from slipping away at a point where, if the traffic be not completely stopped, it will be worked with the greatest inconvenience?

Must the traffic be stopped for a short time, to prevent its being stopped for a much longer time? If so, is not this an objection which would be fatal to the system?

These questions it is hoped, may be answered, by those who have known the difficulty arising from slips and accidents on locomotive lines.

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* Vide Minutes of Proceedings, 1845, p. 144.

In this Paper some of the most important subjects for consideration, respecting the mechanical difficulties, the method of working, the first cost, and the cost and facilities of working and maintaining the atmospheric system, have been suggested, with the earnest desire of excluding all prejudice, and even those opinions, which an examination of the working of most of the railways of England have induced.

This has been done with the hope, that the engineers, whose attention has been so directly called to the subject, as to induce them to adopt or reject its application, will state the facts and arguments, which have influenced their judgment, and that thus a discussion may arise, not among the patentees, the patrons, nor the inexperienced and fanciful opponents of the atmospheric system, but among those, who from their talent and experience are entitled to be ranked among the eminent, and who will, by influencing the majority of the profession on this important subject, prove themselves the leading members of it.

Mr. R. Stephenson regretted not having been present when Mr. P. W. Barlow's Paper on the Atmospheric system was read, he however gladly availed himself of the present opportunity of commencing a discussion on the question, before the Institution of Civil Engineers, where, he must be permitted to say, he thought the real advantages and defects of the system would be, at least, as well understood, and would probably be more dispassionately discussed, than in a Committee of the House of Commons. The grand object of the discussions at the Institution must be the elucidation of the truth, and although he confessed he was a partisan, yet he hoped to be able to review the subject impartially, and if during the progress of the investigation, it could be shown that his opinions were erroneous, he would gladly repudiate them.

Mr. Stephenson's report on the Atmospheric Railway system, had been frequently referred to in the Paper. That report was, with the exception of Sir John Macneil's, the only one published in England, in which the question had been examined on all its points, with a view to the application of the system to a long line of railway. He had been engaged by the Directors of the Chester and Holyhead Railway, to investigate the feasibility of adopting that system of traction; he accordingly proceeded to Dalkey, and although upon his personal inspection, his own conclusions had been arrived at, he deemed it expedient to institute the series of experiments, which had been tried by Mr. Berkeley and Mr. Marshall, and which were recorded in the Report. It had been stated, that those experiments were based on erroneous principles, and that thus under false impressions he had arrived at such an adverse opinion as he entertained. He claimed attention to one or two points of his Report, previously to entering upon the discussion of Mr. Berkeley's Paper.

It would be recollected that in the Report it was stated "that it was evident, the maximum velocity of the train was determined by the proportion existing between the areas of the air-pump and the tube, and the velocities of the air-pump piston and the friction in the tube must obviously be in the inverse proportion of the squares of their diameters. For example, at Kingstown, the diameter of the air-pump piston was 67 inches, and that of the tube-piston was 15 inches; the velocity of the former was 253 feet per minute, or nearly 3 miles an hour, and hence the utmost velocity attainable by the latter would be about 60 miles per hour." This being admitted, it was evident that motion might commence as soon as the air was sufficiently exhausted in front of the piston, to cause an excess of pressure of the air on the opposite side, or behind the piston, "when the train after starting, would acquire an accelerating motion until the maximum velocity was attained which would then continue uniform;" or "until the space passed over by the piston in the tube, during a single stroke of the air-pump, should equal the content of the air pump." "Hence, whether the train were great or small, the maximum velocity attainable would in all cases be the same."

The method of performing the experiments was, by dispatching the carriages from the bottom of the incline as soon as a vacuum was obtained, which was sufficiently high to accelerate the train to such an extent, as that a balance should be established between the power of the engine and the resistances, at the commencement of the last incline, over which it was designed that the mean velocity should be taken. The velocity of the train and the alteration of the barometer were carefully noted, and as soon as the mercury in the barometer became stationary, it was assumed, that the maximum mean velocity had been attained over any one place, where it had continued so. That position was in fact the basis of the experiments; it had however been questioned by Dr. Robinson, but with all due deference to the Doctor's opinion, Mr. Stephenson must still maintain the correctness of his position and the accuracy of his deductions; and on these grounds. That when, by the action of the air-pump, a vacuum was maintained in the tube, at exactly the same rate, as the united effect of the leakage and the motion of the train, exerted their influence to destroy it, or that, a balance was established between the power and the resistance, it was inevitable, that the height of the barometer must remain perfectly stationary. This being the fact, it was equally certain that a stationary barometer indicated the exact balance.

An accelerating motion would result from power being accumulated before the starting of the train, which would be the case until this was all given out; but during that time, the barometer, as shown in some of the experiments, would be lowering.

Accelerating motion would also take place, if the air-pump exhausted the air from the tube faster than the motion of the train and the leakage followed it up. In that case the barometer must necessarily rise. These positions did not in any way interfere with the truth of the previous statements, that when the leakage and the motion of the train, exactly balanced the effect of the pump, the barometer must be stationary. They were anticipated by Mr. Stephenson, who had given instructions that the experiments should be so arranged that they might be avoided. This would be evident from an examination of the circumstances of the whole of the experiments.

In the recent Parliamentary inquiry Dr. Robinson had given an opinion which demanded special notice. He was asked,—

" 710. Q. Then you do not agree with Mr. Stephenson in his Report that the maximum velocity has been arrived at? He replied,—

" A. Certainly not. Mr. Stephenson lays down in that Report, an extremely singular criterion of the maximum velocity being arrived at. In that, or in his evidence before the Croydon Committee, he states, that he is certain the maximum velocity was attained, because the barometer-gauge did not rise; now an extreme case will show, that that test is incorrect. Suppose the tube were perfectly freed from air—perfectly exhausted,—then the piston would sustain, on one side, the full atmospheric pressure of 15 lbs. to the square inch, and no pressure on the other side; it is clear, that the motion of the piston and the train, would go on accelerating, because there would be nothing to check it. It would go on accelerating, as long as the piston was in the tube, until the resistance of the air to the train, balanced the pressure. Now you see that, in this case, the gauge must remain at 30 inches, notwithstanding the extreme acceleration. That will show at once, that the stationary condition of the gauge has nothing to say to the velocity. The real criterion of the maximum velocity being attained is, that the difference between the external pressure of the atmosphere and the pressure of the air in front, shall be greater than the amount of the resistances to the motion of the train. So long as that is the case, so long the motion will continue to be accelerated, even though the gauge may be sinking. The sole argument by which Mr. Stephenson endeavoured to prove, that the maximum velocity was attained, viz., the stationary condition of the gauge, is therefore an evidence, that the motion was still accelerating, unless it be also shown that its height was such, as to indicate a pressure equal to the resistance."

Now Mr. Stephenson contended, that the position of a perfeet vacuum assumed by Dr. Robinson was an impossible case, and must have been used merely for the sake of argument. It was not a sound practical illustration of a true theory, inasmuch as it involved some circumstances which were not possible, and excluded others which had a positive existence, and however perfect the argument might be as a piece of abstract reasoning, it was irrelavent to the practical discussion of the subject.

Mr. Samuda had stated, that the vacuum required by the train of 26·5 tons was only 15 inches; now the fact was, that when the balance was established, a vacuum of 18·5 inches was indicated, and the barometer had gradually risen, instead of fallen; so that Mr. Stephenson contended, the practical fact was more valuable and trustworthy than any calculations, the data for which were not given, and which must at best be extremely uncertain.

Mr. **Bidder** concurred entirely with the principles so clearly laid down by Mr. Stephenson, and gave a practical illustration of it, by observing that the case propounded by Dr. Robinson had reference to a tube of a limited length, whereas the principle should be tested by a continuous series of tubes.

Assuming the engines to be capable of exhausting the tube at a pressure, adequate to any given load, together with the average leakage, at a velocity of 50 miles per hour; then in the case of a train proceeding at an average speed of 40 miles per hour, the effect would be to rarefy the air to a greater degree, and an acceleration of the speed of the train must necessarily ensue, and on the basis of the resistance being the same at all velocities, this would continue until the velocity of the train was increased to 50 miles per hour, at which point the barometer would become stationary, and the velocity of the train would have become steady. Arguing on the more practical hypothesis, of the resistance from atmospheric pressure, increasing with the velocity, the adjustment between the velocity of the train and the rate of exhaustion, must take place at some intermediate velocity.

It was not possible to conceive the occurrence of this equilibrium of velocity without an equivalent steadiness of the barometer; and this balance, Mr. Bidder contended, was clearly established in Mr. Stephenson's experiments on the Dalkey line.

Mr. **Scott Russell** said it had been stated at a previous meeting* that Dr. Robinson, of Armagh, had made abstruse mathematical calculations on the nature of the vacuum, as applied to the production of mechanical power, and the result of these investigations was, that the power exerted in creating the vacuum, and the power usefully employed on the load, were—friction and leakage apart—precisely equal. This statement was made, as a proof of the scientific accuracy of the principle of the Atmospheric system.

At the same meeting it was also stated, that other calculations had been made, which did not show any such equality of the useful effect to the power employed, but on the contrary a great deficiency.

From Dr. Robinson's examination, before the Committee of the House of Commons, it did not appear, that he made any other than an elementary calculation, from which he had entirely excluded the effect of temperature on the column of air. Now that was a most important element in the calculation, essential even to the result, but which had been neglected. It was shown that there were ample means, on scientific grounds, of arriving at an appreciation of the effect of this change of temperature, and that it was so great, that 180° of elevation of temperature gave the pump more work to do than it otherwise had, to the amount of 37 per cent. of the power necessary to condense and eject the rarefied air. This enormous discrepancy had, it would appear, been omitted from the calculation, and hence Mr. Russell contended, that, as scientific evidence, it must be examined very carefully before it could be admitted. The remarks, on the principle of Mr. Stephenson's experiments, were equally erroneous. The opinions on the Atmospheric, as compared with other systems, had also no better basis than the state of railways and locomotive engines in 1834, and were therefore inadmissible. As practical evidence the opinions so based, could not be received.

At a meeting last session† Mr. Bergin stated that "the temperature of the air on being expelled from the pump, was upwards of 200° Faht. Poisson's formula for calculating the temperature under similar circumstances, would have given 343° Faht." Now Mr. Russell contended, that so serious an augmentation of temperature was a material condition for consideration in a mathematical investigation, as the expansion of the air from the heat was an element of serious practical loss.

Mr. Russell was persuaded that if this had been considered by Dr. Robinson, he would have arrived at different conclusions.

Mr. Stephenson's experiments appeared to Mr. Russell to be based on sound principles, and to have been very carefully tried; the deductions from them might, in his opinion therefore, be received as correct.

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* Vide Minutes of Proceedings, 1845, p. 149.

† Ibid., 1844, p. 276.

Mr. **Samuda** said, that his views were known to be entirely at variance with Mr. Stephenson's, and after having given the most careful attention to that gentleman's remarks, and to his published report, he saw no reason to change the opinions he had expressed.

Mr. Stephenson had referred to the principles which governed the speed, on the Atmospheric Railway, and added, that it had been stated he had based his report on wrong premises; Mr. Samuda did not assert that; on the contrary, he entirely agreed with the principles laid down in that report, with reference to the attainable speed; he also agreed, as to the amount of leakage, shown in the Table No. 4; indeed, the quantity there given, was less than that observed by Mr. Samuda in previous experiments. But he disagreed with Mr. Stephenson in the conclusions he had drawn from these premises.

An example might be taken from one of the experiments, given in the table of velocities, and it could there be shown that different conclusions might be arrived at. The report said, in referring to train No. 4:—"The 4th train was 26·5 tons weight, and started with a vacuum of 8·7 inches: the velocity and pressure attained a steady maximum velocity of 34·7 miles per hour, and 18·5 inches of mercury." Then followed the conclusion, based on this experiment,* where the calculated maximum velocity was tabulated at 49·3 miles per hour, and the practical maximum velocity was stated to be 34·7 miles per hour; the difference of 26 per cent. being taken as the loss of the system. Mr. Samuda objected to this conclusion, and the apparent discrepancy between the calculated and the practical results, which must, in his opinion, have been arrived at from an erroneous reading of the experiment. If a summary was taken from the recorded experiments for this train, it would be found, that the train was started from No. 5 post, being one-eighth of a mile from the Kingstown end of the line;—that it was allowed to move, immediately the vacuum rose high enough to overcome the resistance, and it began to move when the barometer indicated 8·7 inches; that in passing from the posts—

			Miles per hour.	Vacuum.
No. 15 to 19, both inclusive, or 1 furlong, its speed was			20·16	16·7
20 to 24,	,,	,,	23·70	17·2
25 to 29,	,,	,,	25·84	17·5
30 to 34,	,,	,,	28·62	18·0
35 to 39,	,,	,,	29·62	18·5
40 to 44,	,,	,,	31·62	18·5
45 to 49,	,,	,,	35·48	18·5
50 to 54,	,,	,,	37·08	18·5

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* Vide Stephenson's Report, p. 11, Table 4.

At that point the piston left the vacuum tube, and the rest of the journey was performed by the momentum previously acquired, and the train then travelled, from posts No. 55 to 59, or 1 furlong, at the rate of 36·14 miles per hour. The train, then, was started from a state of rest, breasting an inclined plane of 1 in 166, and when the gravity and friction were only just exceeded by the vacuum in the tube, its velocity was shown to be accelerating, from first to last. That in fact, there was a regular progressive increase of velocity up to the moment of the piston leaving the tube, when the propelling power ceased. That the acceleration (as given in the table, column 4) was, while passing

						Miles per hour.
From the	1st	furlong (posts 15 to 19)	to the	2nd,	equal to . . .	3·64
„	2nd	„	„	3rd,	„ . . .	2·
„	3rd	„	„	4th,	„ . . .	2·92
„	4th	„	„	5th,	„ . . .	1·
„	5th	„	„	6th,	„ . . .	2·
„	6th	„	„	7th,	„ . . .	3·86
„	7th	„	„	8th,	„ . . .	1·60

Now instead of the conclusions drawn from this experiment, that the velocity and pressure attained a steady maximum of 34·7 miles per hour, and 18·5 inches of mercury, Mr. Samuda thought, the correct reading of the experiment would show, first, that the train never acquired a steady maximum, and secondly, that the velocity it did acquire, was considerably greater than 34·7 miles per hour.

It should be further observed, that these experiments were taken over a portion only of the length of the line. It had also been shown, that a maximum speed was never acquired, indeed it could not have been attained, even had the train been started from the extremity of the line; but the mode adopted, of starting from a point 220 yards from the end, materially influenced the speed attained. Again, the trains were not held back while the vacuum was forming; but they were allowed to start, when it was less than half formed, and thus a greater distance became necessary for them to reach their full velocity.

A parallel might be drawn, by comparing the treatment of this train with the result that would be obtained, by placing a locomotive engine, on the same incline, with a similar train, lighting the fire, opening the steam valve, and allowing the train to move forward, so soon as the steam had risen sufficiently just to put the train in motion; then to leave for the locomotive engine the double duty of getting up its steam while in motion, and at the same time accelerating the train.

It must be obvious, that starting without any excess of power to accelerate the load, a much greater distance must be passed over before the vis inertiæ was overcome, and in the supposed case of the locomotive, and in the experiment made with the Atmospheric train, not only was the latter taxed to overcome the vis inertiæ, without an excess of power, beyond that required to propel the train, at its ultimate velocity, but it had to commence its operations, with a power wholly insufficient to obtain the ultimate required effect.

Mr. Stephenson had laid great stress on the correctness of his conclusions, because the speed of the train was uniform, while the barometer remained stationary, but Mr. Samuda contended, that these two points of reliance did not exist; that the barometer was noted in the experiment as stationary, while the speed of the train was shown as accelerating from 29 miles per hour to 37 miles per hour, and that therefore either the barometer, or the speed, was not correctly noted.

It might be imagined, that the error must have occurred in reading off the barometer, as its variation would be very gradual, and from the motion of the carriage, and the time for making the observations being only 40 seconds, any alteration of the mercury would be appreciated with difficulty, and it was somewhat surprising, that Mr. Stephenson should have relied on the barometer as an index of the maximum velocity, when the fact was contradicted by the accumulation of the speed of the train. The probability of error in noting the vacuum gauge, was greater than could occur in reading off the seconds, while passing the posts.; and for this reason—the progression of the seconds on a dial, was tolerably distinct and regular, so that one observation corrected another, and it was also unaffected by the motion of the carriage, or other causes. Now, the progressive rising of the mercury in the gauge was slower, as the train increased its speed, and after a tolerable speed was attained, it would not vary an inch in a mile; it was very much affected by the motion of the carriage, and varied, with every variation of speed of the engine, so that, it was impossible to make observations on it, which would correct each other, unless more time was allowed than was occupied by the whole journey from Kingstown to Dalkey.

Moreover, even if the gauge had been stationary at 18½ inches, it would have been no indication that a steady maximum was attained; for the weight of that train (26·5 tons) would only require 15 inches to keep up the motion, after the vis inertiæ had been overcome, and therefore the gauge must have fallen to that point, before the maximum speed could have been attained.

Mr. Samuda would rather have confined his present observations to this point, as being the foundation on which Mr. Stephenson's conclusions were based, but he could not omit noticing one great element of error, that he conceived Mr. Stephenson had introduced into his calculations. All his deductions were drawn from experiments, in which the vacuum ranged from 18 inches to 24 inches of mercury, so that, even if they had been correctly drawn, they would not have furnished any true indication of the economy of the atmospheric system, as the degree of vacuum at which it was proposed to work ordinary trains, was about 15 inches of mercury; for the waste of power augmented exactly as the degree of vacuum was increased.

It was well known, that a perfect vacuum was not attainable, and if the apparatus was taxed, to the extent of approximating, as closely as its capabilities would admit, it was evident, that the effort would, at every step, become more difficult, and for this reason it would be injudicious to carry it so far. If it had been intended to work the system to 24 inches or 25 inches, as presumed by Mr. Stephenson in his comparison, his mode of treating it would not be so objectionable; but it had been ostensibly put forward, that the working point was calculated at 15 inches or 16 inches; that the scale of the apparatus, for performing the traffic of a line of railway, was based on this supposition, and any point beyond this would be used only on emergencies; that, in fact, the use of higher degrees of vacuum would be the exception, rather than the rule, and the use of them towards the upper end of the scale should be discountenanced altogether, in an economical point of view. The employment of these very high degrees of vacuum ought therefore to be discarded from the calculation, and the apparatus should not be taxed to a degree, that experimental researches and great emergencies could alone justify; the results would then show, according to Mr. Stephenson's own observations, a striking superiority due to the Atmospheric Railway, with regard to the transmission of power, when compared with locomotive engines.

Mr. Stephenson, in answering Mr. Samuda's remarks upon the experiments in the Report, maintained it must be received as an axiom, that an uniform height of the barometer implied a state of equilibrium, between the power given out and the total resistance.

Dr. Robinson assumed a case which had no existence in Nature, and ought not to be introduced into a discussion, where true results were sought for, and in which party feeling and prejudice should be entirely laid aside. Mr. Stephenson preferred examining a real, rather than a supposed case.

Mr. Samuda had, in his remarks, cast some doubt, not so much on the principle of the experiments, as upon the accuracy of the deductions from them, and had attempted, by grouping the observations of the velocities, to prove contrary results to those arrived at by Mr. Stephenson, which were, however, he contended, substantiated by the recorded steadiness of the column of mercury.

The comparative value of the two columns in the table of the velocities and the height of the barometer, could not be fully and fairly appreciated, unless the nature of the instruments, and the method of using them, were clearly understood.

The instrument by which the velocities were observed, consisted of clockwork, with a revolving dial, larger than a watch, the periphery of which was subdivided so as to show quarter seconds, with lines radiating from the centre for the seconds.

At the time of passing any point, by touching a spring, a small pen or cup of ink, was projected so as to leave a mark on the dial. The posts along the line, were placed at distances of 2 chains, or 44 yards apart.

Now, with this instrument, three sources of error existed. First. The action of the clockwork was a succession of beats, and not a regular progressive movement; which, in marking minute intervals of time, became an important consideration. Second. The breadth of each of the lines made by the pen, was frequently more than a quarter of a second, and if the finger of the observer rested for any time on the spring, a line of double thickness resulted, or two lines were made. Third. It was almost impossible for the observer to note the time of passing a post, with perfect accuracy, or without committing an error of a quarter of a second.

These sources of error might either neutralize each other, or might mutually add to the total inaccuracy.

From this it would be evident, that perfect accuracy, in noting the velocity, was scarcely possible, and the probable amount of the error might be appreciated from the fact, that an error of a quarter of a second in passing each of any two posts, at the velocity of 36 miles per hour, would occasion a difference of 7·3 miles per hour, as was actually shown in the experiments in the Report.

With regard to the height of the barometer, he contended, from his own experience, and from that of Mr. Bidder, Mr. Marshall, and Mr. Berkeley, that it could be observed with perfect accuracy, when the balance between the power and the resistance was established. This was rendered more certain and conclusive, when several observations were made while the mercury remained at the same height, as in several of the recorded experiments.

For these reasons, he concluded, that the observations of the dial, could not be so precise, as those of the column of mercury. In fact, when recording the velocities, many discrepancies appeared, which had no doubt resulted from these causes; these discrepancies he preferred recording, because he considered, that the experiments should be shown to the world, exactly as they were taken, and that the experimenters had strictly attended to the instructions received from him, before commencing their operations.

The principle of grouping the observations adopted by Mr. Samuda, he generally concurred with; but, in this particular case, where the error was so large, he contended, that a sufficient number of observations had not been taken to eliminate it. He had previously shown, that an error of 7·3 miles might exist, which by grouping five observations would be only reduced to 1·5 miles per hour, which was still a great error.

To show the impossibility of obtaining a correct result from this column of velocities, he would take the case which had been quoted, where—

From the 45th to 49th post, velocity 35·48 per hour
And from the 50th „ 54th „ „ 37·08 „

giving an acceleration of 1·6 miles per hour.

Now, if the same figures were differently divided, the result would be—

47th to 50th post, velocity 37·1 per hour
51st „ 54th „ „ 36·3 „

showing a retardation of ·8 mile per hour. Thus, from the same figures, but by grouping them differently, acceleration or retardation could be produced.

He would ask, whether this column of figures, and this method of treating them, could be considered sufficiently correct, to substantiate the objections which were raised, to the sound principle, which he contended, had been adopted as the basis of the calculations in the report. He alluded to what he must consider an axiom; that with a power working uniformly, and a perfectly steady height of barometer, a maximum velocity was at one moment attained, and though some acceleration might, and he contended, should be observed, it was entirely due to a reduction in the losses involved in the system, which were the consequence of a certain length of pipe and valve, being between the train and the power, which length, and consequently the losses due to it, was continually being reduced during the passage of the piston.

His sole object in arranging the experiments was, so to proportion the starting vacuum, to the weight of the train, that the power accumulated should all be given out, before arriving at the last plane, and that thus a balance might be established between the resistances and the power of the engine in operation during the motion of the train;

when, he anticipated, and held firmly to the fact, that a steady height of the barometer must result. He considered the descending incline, at the commencement of the line, only introduced complexity; he had, for that reason only, directed that the trains should be started from the bottom of the incline, and he contended, that the mode, which had been adopted, was the best for exhibiting the mean velocity which the power of the engine could maintain.

Mr. Samuda's comparison of the Locomotive to the Atmospheric system was not analogous, unless the fire was lighted under the boiler of the Dalkey engine, at the same time, as the fire in the locomotive was lighted. If a comparison was instituted between the two systems, it should be by running a locomotive and train for three minutes, along the Kingstown line, and then proceeding onwards to Dalkey with the acquired velocity, which would then be more nearly assimilated to the power gained by working the Dalkey engine for that space of time, and creating a partial vacuum, before starting the train.

Mr. **Vignoles** thought that the discussion would have been more satisfactory if, instead of turning upon points of theory, it had been directed to the questions of the comparative cost and the commercial benefit of the two systems, which were the most essential considerations.

He must, however, uphold the correctness of Mr. Samuda's views, and of his comparison of the locomotive and the fixed engine. It must be evident, that the entire length of the line and the duration of the journey, were much too restricted to exhibit the full advantages of the system, or to enable the engine to develop advantageously its full powers, which could not be done under a length of at least 3 miles or 4 miles. It was not, he submitted, judicious to diminish the length of a run which was already too short, and was accomplished at too great a velocity, to insure accuracy in the observations of the times of passing posts placed only 2 chains or 44 yards apart; in which distance he did not think it was possible to observe correctly.

Mr. **Berkley** said, Mr. Stephenson had strongly urged upon him to banish all prejudice, and to use every means of ascertaining the truth, and he had gone to Dalkey with a determination to adhere rigidly to this main instruction. The truth, however, of the experiments appeared to be doubted; he therefore felt called upon to state the particular instructions given, how he had understood them, and in what manner they had been carried into execution.

He was instructed to note, with great care, the exact height of the barometer, and also to observe the velocities attained; these he had done, feeling the great importance of the experiments, and he could state, that while the barometer was rising, particularly in the early stage, when the density of the air was great, there was considerable oscillation in the column of mercury; this oscillation gradually decreased with the progress of the train, or the rising of the barometer, and that when a balance was established between the power exerted and the resistances, the indications were read off with certainty and perfect accuracy. He found in his rough notes that they were observed to half a decimal. A perfectly stationary height of the barometer was also observed when the train was not in motion, but when the power of the engine had raised the column of mercury to its extreme height. These cases, he contended, were perfectly analogous, inasmuch as, in the former case, a balance was established between the power of the engine and the resistances, consisting of the leakage, the friction of the air in the tube, and that due to the motion of the train; while in the other, the balance was equally established between the power and the resistance, which consisted of the leakage and the friction of the air only.

He corroborated the statement made by Mr. Stephenson, with regard to the error in observing the velocities, which it was impossible entirely to eliminate, and which was done to a very small extent by the grouping which had been adopted.

He contended that a sound principle should not be upset by arguments, the basis of which was erroneous, and which could be so turned as to suit the views of either party who used them.

Mr. Berkeley was further instructed, to start the trains with such a vacuum, that a steady height of barometer should be observed over the last plane; and it would be seen from the experiments, that considerable pains had been taken to obtain this result: for, when it was found that by starting a light train with a high vacuum, the carriages overran the power of the engine, as was shown by the falling of the barometer, the same, or a rather heavier train, was started with a lower vacuum, in which case a steady height of barometer over the last plane was attained.

It had been stated, that the trains were started as soon as the vacuum in the tube was so high that they would move. This statement was erroneous, for in every case, with one exception, the breaks were held on until that vacuum, which was considered necessary, was attained. The height, in two cases, reached 24 inches, and throughout the experiments was at almost all intermediate heights; in fact, the height of the column of mercury, at starting, was proportioned to the weight of the train.

It appeared to him an axiom, that a steady height of barometer was a certain index of an equilibrium, between the velocity of the air extracted by the pump, and that which entered to fill up the space. He must remark that Mr. Stephenson had not dealt with perfect justice, in treating the maximum mean velocity, which would be maintained in practice. He had treated it liberally, and had fixed it too high. This might be illustrated, by representing the power acting uniformly, by 10; the losses due to every half mile of pipe and valve by 1. Then, if the train was 1 mile from the pump, the losses due to that length would equal 2, leaving 8 for the velocity of the train. If the train was advanced to within ½ a mile of the pump, the losses due to that length equalled only 1, there remained therefore 9 for velocity, and when the train was close to the pump, the whole of the power, (10,) was employed for velocity. The fair maximum mean velocity of the length must therefore be represented by 9, but if the train ran into a pipe 3 miles in length, every ½ mile representing 1, 6 parts of the power would be employed in overcoming the losses of leakage, &c., due to that length, and only 4 parts be left for velocity; in that case the maximum mean velocity would be 7.

It must be evident, that as there was a difference in velocity of 7 parts and 9 parts of the power, between the circumstance of lengths of 3 miles of pipe, and of 1 mile of pipe, Mr. Stephenson had not treated the practical application with rigid justice, inasmuch as he had taken the maximum mean velocity, which was maintained over a distance of only 24 chains, at the end of the pipe. The velocity had therefore been taken much too high, for any practical application of the system to the intended length of pipe.

Mr. **Barlow** stated, that whatever might be the length of the pipe, it was evident that from a diminution of the leakage, the speed of the train would be accelerated as it approached the extremities; and, if the pipe was sufficiently long, the whole power of the engine would be consumed in pumping out the leakage alone.

He therefore considered, that as Mr. Stephenson had used the velocity near the end of the pipe, his calculation as to the mechanical effect of the Dalkey Line was rather favourable to it, than otherwise.

The expression "maximum uniform velocity" did not in his opinion indicate precisely what it represented, in the investigation of Mr. Stephenson, and some misunderstanding might have resulted, as it was in fact treated as the speed due to the average vacuum, or leakage of half the pipe, and consequently the longer the pipe, the less would be the average speed.

Mr. Barlow repeated, that from a careful examination of Mr. Stephenson's deductions he considered, that as the speed used was near the end of the pipe, it gave a favourable representation of the average speed, and consequently of the mechanical effect of the Dalkey Line, with the degrees of vacuum used in the experiments. If the experiments had been made with lower degrees of vacuum, the amount of lost power would, of course, have been less.

Mr. **Charles Hutton Gregory** stated, that he had made a series of experiments, on the working of the atmospheric system, at Dalkey, and had, conjointly with Mr. Bergin and Captain Nelson, R.E., carefully noted simultaneous observations, on the comparative vacuum at the engine and in the piston carriage, as well as on the velocities of the engine and the train. He could not agree with Mr. Berkeley, as to the facility of observing, to a decimal, the indications of the barometer, as the oscillations, during the experiments tried by him, were so great, that it was difficult to note them with any accuracy within ½ an inch. He considered that a closer result might be obtained, from the instrument used by Mr. Bergin for calculating the velocity of the train. He had observed a continual increase of speed in most of the trains, up to the period of their leaving the tube, when the barometer exhibited either practically an uniform force, or one slightly declining. He thought that the velocity of the engine had generally been assumed too great. He had noted it at 21 or 22 revolutions per minute, instead of 23 revolutions, as had been generally assumed, and he believed that any deduction must mislead, in which the observer omitted to note accurately the number of revolutions of the engine, during the experiments. He would submit, that the Dalkey Line did not afford the means of deducing the maximum speed or power of the system, any more than a line of similarly steep gradients, and sharp curves, could develop in as short a distance, the available useful power of the locomotive.

Mr. **Samuda** allowed Mr. Stephenson credit for giving the clearest instructions as to the mode of preparing the experiments, and did not question the honesty of the manner in which those instructions were carried out by his assistants; but he contended that erroneous deductions had been drawn from the results. He was the more surprised to hear Mr. Stephenson objecting to Mr. Samuda's summary of the experiments arrived at, by grouping the results of every successive five observations, as on reference to Mr. Stephenson's report,* it would be seen he pointed out, that a strictly correct result would follow from adopting such a course. "There is an irregular variation in some of those velocities, which is owing to the difficulty of exactly measuring such small intervals of time; but in the deductions from these experiments, an average of several intervals has always been taken, which affords a strictly correct result."

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* Vide Stephenson's Report, page 8.

It would be unnecessary to repeat more emphatically, that it was absolutely impossible to arrive at maximum velocities on the Dalkey line by adopting the course of experiments that had been instituted —that a reference to the space moved over by locomotive trains, would serve as a comparison, to show that it was unreasonable to expect the result should be otherwise.

Mr. Samuda had a return of some observations taken on the London and Birmingham Line, whence it appeared, that on a level, an engine (No. 35) drawing 12 carriages, required 5′ 42″ and passed over 1½ mile, to obtain a speed of only 23½ miles per hour. Another observation, also on a level, showed that 2 engines drawing 13 carriages (equal to 6½ carriages per engine,) required 5′ 40″, and passed over 1¾ mile, to obtain a speed of 30 miles per hour.

These were comparatively low speeds, and yet more distance was moved over to obtain them, on a level and straight line, than had been assumed as sufficient to develop a full speed, on the curved and inclined line at Dalkey.

His own observations, on the Great Western Railway, made April 19th, showed that though a speed of 25 miles to 30 miles per hour was obtained in a comparatively short distance; yet it required 6 miles or 8 miles to be passed over before the train arrived at a speed of 53 miles per hour.

It had been stated, that maximum velocities had been obtained, because although an acceleration had been shewn by Mr. Samuda to exist, that acceleration was due to the diminished leakage, as the train moved up towards the engine, and shortened the length of longitudinal valve between it and the pump. Mr. Samuda could not omit, now that it had been admitted the velocities of the trains were accumulating, to show that the cause assigned was insufficient to maintain the position taken, and he referred to this, particularly, because he found Mr. Stephenson, in his evidence before the Select Committee of the House of Commons, had availed himself of this point, to account for the inconsistency between his observations and conclusions.

Mr. Stephenson attributed nearly ⅘ths of the leakage, to the connecting pipe and air pump, and he had added for supposed leakage, when the train was in motion, a sum greater than the entire leakage when at rest; in the proportion of 14 to 8. This was arrived at by the difference between what was called the calculated and practical velocity (page 11), which Mr. Stephenson stated at page 13 was owing to additional leakage while in motion, which he had no direct means of measuring.

This last mentioned portion, or $\frac{14}{22}$nd parts of the whole, would therefore exist as much when the train was close to the connecting pipe, as when it was far off (if it existed at all) and so would three-fifths of the remaining 8 parts—5 parts. There were therefore 19 parts out of 22 from which the advance of the train would cause no reduction.

Referring then to the train already alluded to (No. 4), it would be found travelling 37 miles per hour for the last furlong in the pipe. The greatest speed the dimensions of the pump could have given, (exclusive of all deductions for leakage,) was about 54 miles per hour, when making 22 double strokes per minute, which was the greatest working speed.

The deficiency of 17 miles per hour, or about 31 per cent., would be thus divided,—19 parts, or nearly 27 per cent. uninfluenced by the forward motion of the train—leaving only 3 parts, or 4 per cent. acting to accelerate the speed.

This 4 per cent. would represent about 2 miles per hour—that is to say, that the speed, from first to last, would be increased 2 miles per hour, from the diminished leakage; but the train had been shown to have accumulated nearly 9 miles per hour, while passing over only 3 furlongs; it was therefore clear that the diminished leakage did not account for the acceleration that was evidently taking place.

Mr. Scott Russell stated, that the arguments adduced against the experiments of Mr. Stephenson, were not such as to carry conviction to his mind.

Mr. Samuda had attempted to establish a comparison between the Atmospheric Railway in starting, and the case of a locomotive gaining its maximum speed. There was, however, an essential difference, inasmuch as the latter might be considered rather a chemical than a mechanical question. The position was, how soon after starting, would the fire in the boiler arrive at the best state of combustion, so as to attain the most rapid generation of steam? now this example must be admitted by every practical engineer, not to be an analogous case.

Mr. Gregory had stated, that when making his observations, much oscillation of the column of mercury occurred, and therefore, he did not agree in the possibility of the attainment of a steady height of the barometer, in the experiments. This, in Mr. Russell's opinion, merely showed, that the proper precautions, which had been taken in one case, had been omitted in the other; for the oscillation could easily be prevented, and in experiments which Mr. Russell had made, under similar circumstances, he had found no difficulty in obtaining a steady mean result from the column of mercury.

There was another argument adduced against the locomotive, which was not sound. The locomotive was said to require more force, in the ratio of 2 to 1, to propel it, than an equal weight of carriages. This might be the case at very low velocities, though Mr. Russell thought that even for them, it was overrated. He had made observations on inclines of 1 in 100, and found that the resistance of a locomotive engine, and that of a light train of four carriages, was, weight for weight, at 28 miles to 30 miles per hour, precisely equal. These experiments coincided with those made by Mr. Woods, and the Committee of the British Association, which showed, that the resistance to trains, at high velocities, was much greater than was generally admitted.

It was plain therefore, that the resistance of the locomotive had been overstated, and that to the trains of carriages on the Atmospheric Railway had been understated. He contended therefore, that the arguments on that point could not be received as conclusive.

Mr. VIGNOLES protested against Mr. Russell's strictures upon the opinions and evidence of Dr. Robinson, which he contended should not have been commented upon so strictly, nor should the experiments of the British Association, which were instituted for entirely different purposes, have been introduced into the discussion.

Mr. LOCKE corroborated Mr. Russell's statements as to the results of the experiments instituted by the British Association. He was not only a member of the Committee, but his own experiments on the subject, made on other occasions, convinced him of the correctness of the results obtained. He contended that as the opinions, which had been commented upon by Mr. Russell, had been given before a Committee of the House of Commons, they might with propriety be examined at a meeting of the Institution, and if they were not sound, a service would be rendered, by the application of any test, which would demonstrate the amount of credence which was to be placed in their accuracy.

Mr. C. MAY thought, that in the present state of the atmospheric system, no definite results could be arrived at, even from the most prolonged discussion. His personal inspection of the Dalkey line had convinced him, that a correct opinion could not be formed, from the operation of the system upon so short and peculiar a line, where the permanent way was very indifferent, the construction of the pipe was defective, the engine was not properly adapted for its work, the gradients were unnecessarily heavy, and the curves were so sharp; as a very cursory examination sufficed to show, from the abrasion of the rails, that a large proportion of the power was expended, in overcoming the friction of the flanches of the wheels against the inside of the curves. He contended, that no correct opinion as to the general application of the system could be formed, until it had been well tried upon a properly-executed line of 6 miles or 7 miles in length.

Mr. **Fairbairn** said he had been requested by the Council of the Manchester Philosophical Society, to examine the Dalkey Railway, with a view to reporting upon its efficiency. He had not however been able to devote sufficient time to the subject or to undertake any experiments; but from what he had seen, he was of opinion, that even in its present state, the system was applicable to certain peculiar situations, and judging from the rapid improvement in locomotives, within a few years, it was only fair to argue, that great changes in the system would be made, which would probably extend its applicability; at present he did not conceive it was calculated to supersede traction by locomotives on long lines with heavy trains, however it might be adapted for short lines, with rapidly succeeding trains and a light or equally distributed load on each journey. He entirely agreed in the difficulty of drawing a definite conclusion of the applicability of the system to long lines, from any results obtained upon a line so peculiarly situated and constructed as that at Dalkey.

Mr. F. **Braithwaite** said he had paid attention to the arguments used on both sides of the question, and it was his opinion, that, up to the present time, there had not been afforded any sufficient opportunity for testing practically the working of the atmospheric system. Mr. R. Stephenson's Report did not appear to him unfavourable to the system, but as he had stated, on analysing the first experiments, many of the results appeared so irreconcileable with each other, and presented such anomalies in themselves, that he had determined on further inquiries; these he caused to be instituted with rigid exactness, by confidential assistants, but at the same time with all apparent fairness, and the result was that in his opinion "The atmospheric system was not an economical mode of transmitting power, and it was inferior in that respect both to locomotive engines and to stationary engines with ropes." This brought the question to the point where Mr. Braithwaite would lead it, viz., to the expediency of adopting the atmospheric system upon lines of ordinary construction, and in that view was involved the commercial part of the question, which did not appear at present to have been sufficiently attended to. It was admitted by Mr. Stephenson, "That on some short railways, where the traffic was large, admitting of trains of moderate weight, but requiring high velocities and frequent departures, and where the face of the country was such as to preclude the use of gradients suitable for locomotive engines, the atmospheric system would prove most eligible." Now such situations were not common, and any system which had the appearance of being only adapted for such special cases, should be very carefully analysed, before it was admitted upon the main lines of this country. He much doubted whether the Epsom line, the result of the experiments upon which was so confidently looked forward to by the advocates of the atmospheric system, would be an adequate trial of the capabilities of the system. It was understood that the line was so laid out that if the system failed, it could be worked by locomotives. Now if the gradients were of that easy character, the main part of the proposed advantages, which offered the inducement of greatly diminished earthwork and cuttings, and consequent reduction of expense, to meet the extra cost of the main pipe, vanished. In the consideration of the question this was of vital importance. Other points of great importance would no doubt be brought forward by those who had given more attention to the question of the commercial advantages or disadvantages of the system : that was in his opinion the main point, and to that he wished to direct the attention of the members.

Mr. N. WOOD was not prepared to examine the relative commercial merits of the two systems of propulsion ; he did not think that sufficient experience had yet been obtained of the capabilities of the atmospheric system, to enable an unbiassed judgment to be arrived at. He was however of opinion, that the resistance of the air, in its passage through the tube, would prove a serious obstacle. Mr. Mallet's report suggested the idea, and Mr. Stephenson had confirmed it in his experiments and report.

In Mr. Wood's experience as a miner, he had found it extremely difficult to contend successfully against the effect of this resistance of the air, when travelling along tubes of infinitely greater area than that of the pipe used in the Atmospheric Railway, which was only 15 inches' diameter. He found that lengths of 4 miles to 5 miles were the greatest distances to which he could get the air to travel consecutively. If it was requisite to convey it further, the lengths were divided. He was convinced that the same result would be found in practice on the Atmospheric Railway, even although the exhausting apparatus was much more perfect than that used for mines, and a great power was devoted to the purpose ; still when that power was brought to act suddenly upon so elastic a fluid as air, the difficulties and the resistance would be increased in proportion to the length of tube under exhaustion.

Mr. JOHN TAYLOR coincided in the opinion of the friction of the air within the pipes being a point for serious consideration, particularly when those pipes were extended to a great length. He instanced a case of the ventilation of a mine, where the air was extracted by the power of a water-wheel; as the length of pipe was increased, the speed of the wheel gradually diminished, until the friction of the air completely stopped it, and other means were of necessity adopted for accomplishing the ventilation.

Mr. **Lowe** called upon Mr. Hawksley to lay before the meeting a formula, which had been found extremely correct in testing some experiments on the quantity of gas passing through long mains of pipes, under various pressures and in given times. He believed it to be equally applicable to the question of the resistance arising from the friction of the air in the tube of the Atmospheric Railway.

Mr. **Hawksley** said, that he was quite unprepared for bringing forward the complex calculations mentioned by Mr. Lowe, as he had had no previous intimation on the subject; he would, however, state the fundamental formula for the discharge of air of mean atmospheric density through cylindrical tubes of considerable diameter and length. This formula had been originally designed for other purposes, but was, he found, particularly applicable to the atmospheric-railway question.

The formula was

$$V = 21{\cdot}7\sqrt{\frac{h d}{l + 2 d}}$$

in which V = the velocity of discharge in yards per second.
h = the height, in inches, of a column of water, which would afford a pressure equal to the motive force.
d = the diameter of the tube in inches.
l = the length of the tube in yards.

If the air was of less than atmospheric density, the velocity would be increased, in proportion to the square root of the number of expansions. If the air was of more than atmospheric density, the velocity would be diminished, in proportion to the square root of the number of compressions.

As respected the atmospheric railway, the resistance was greater than that which was due to the velocity of the train, or, in other words, the velocity of the train was less than that which was due to the value of h, in consequence of the introduction of air by leakage, at all points between the place of the train and the discharging extremity of the tube. Moreover, the velocity of the train was further diminished by the increase in the volume of air, consequent on the expansion due to the diminished pressure at the discharging extremity, although the air was there also of less density; for the resistances measured by h were as the squares of the volumes, and only in the simple ratio of the density.

The *application* of the formula was very general; it applied to all perfect fluids, *i. e.* to all which were unaffected by viscidity or chemical affinity.

The result for air of atmospheric density only required to be varied, in the inverse ratio of the square root of the comparative density, to represent the velocity which would obtain in practice from the employment of air of other densities, or of steam, gas, water, or mercury.

Thus, from the formula it would be found, that air of atmospheric density could not be made to flow into a perfect vacuum through a tube of 15 inches diameter and 3 miles long, when propelled by the entire force of the atmosphere, with a velocity greater than 47 miles per hour for

$$V = 21 \cdot 7 \sqrt{\frac{400 \times 15}{(3 \times 1760) + (2 \times 15)}} = 23 \text{ yards per second, or } 47 \text{ miles per hour.}$$

Moreover, if the motive force were only one-fourth of the atmospheric pressure, and the density of the air were only one-fourth of air of atmospheric density, then the velocity would still remain 47 miles an hour; and this would be the velocity of an atmospheric-railway train, when the force required to move the load along a 15-inch tube, of 3 miles in length, was equal to 18¾ inches of mercury, for then it would require 7½ inches of pressure to move the air, and the mean density of the air would be about equal to one-fourth of the density of the atmosphere. But this case supposed the non-existence of leakage; the leakage would detract most materially from the velocity of the air in front of the piston, which would be less than the velocity at the engine, in proportion to the volume which the air, admitted by leakage, might bear to the whole volume extracted by the engine. The train would therefore necessarily proceed more slowly than the air, and if the distance between the engine and the train were very considerable, or the intermediate leakage were great, the train could not be set in motion by the action of the exhausting machinery.

The motion of the train was due to the force h, and was therefore, to a great extent, independent of the power of the engine; for the engine could only produce a vacuum in the pump into which air would flow from the tube, not by virtue of the powers of the engine, but by virtue of the unbalanced atmospheric pressure (h). Hence an infinite power of engine would not make air of atmospheric density move through a tube 15-inches diameter and three-miles long, with a greater velocity than 47 miles per hour.

Another circumstance which interfered with the direct application *of the formula to the* case of the atmospheric railway arose from the variable density of the air in the tube. Near the engine the air was, as already observed, more attenuated than near the train, and would consequently move with less friction. When the train approached the engine, there would also be less leakage, and consequently from the joint operation of these two causes, the velocity of the train would be constantly accelerated; and at the engine, the velocity would be the maximum due to the ratio of abstraction of air, or inversely, the velocity of the train must always (except close to the engine) be less than the velocity of the air at the engine. But it must nevertheless be observed, that the maximum velocity could not be obtained, unless the magnitude of the pump and the power of the engine were adequate to the withdrawal of the air at the maximum velocity, and when its volume should have also attained the highest amount of rarifaction.

The motion of an atmospheric railway train was therefore necessarily peculiar. It commenced at a distance very slowly, and gradually increased, till at length the velocity might be as high when entering a station as was compatible with the safety of the passengers. But this could only be accomplished at great cost, and even then the mean speed, which was all the public cared about, would not exceed that which an ordinary locomotive would produce.

It was also remarkable, that this variable speed was attended with a much greater loss of force, than was due to the mean speed, under which the same distance might be passed in the same time. For the principal resistances within and without the tube, viz. the resistance to the passage of the train through the air, and the resistance to the passage of the air through the tube, both increased as the squares of the velocities.

The formula was not minutely correct, because it did not take into account the peculiar additional resistance produced by the attraction of the particles of fluid to the sides of the tube, nor of the diminution of resistance occasioned by the rolling of the particles on their own centres.

The introduction of these elements would make the formula excessively complicated, without in general making any very sensible difference in the results.

The accuracy of the expression had been variously and frequently tested, as for instance at Birmingham, where there was a pipe for conveying gas, nearly 7 miles long and 16 inches diameter. Also in London, where Mr. Lowe has recently laid down a gas-pipe of 18 inches in diameter and just 1 mile in length. It was roughly predicted from the formula, that the pipe would discharge under a water pressure of 1 inch, 60,000 cubic feet per hour. On trial the delivery was found *to* be *66,000* feet per hour. As the gas was of the density of ·400 (air being 1) the result for air must be multiplied

by $\sqrt{\frac{1}{.4}} = 1{\cdot}6$ nearly;

therefore $V = \left(21{\cdot}7\sqrt{\frac{1 \times 18}{1760 + (2 \times 18)}}\right) \times 1{\cdot}6 = 3{\cdot}47$ yards per second, a velocity very nearly equivalent to 66·000 cubic feet per hour.

In a great number of other instances the formula had been applied with equal success.

The formula was not empirical, but was the result of an independent investigation of principles, and the careful examination of a number of experiments made by himself and others.

From the result of these and other investigations Mr. Hawksley drew the conclusion, that the atmospheric principle was inapplicable to long sections of tube, and therefore was generally inapplicable to the traffic of a long line. He assumed the case of a tube of 15-inches diameter, and took the practical case in which the mercury of the vacuum gauge would stand in the engine-house at 24½ inches, and in the piston carriage at 20½ inches. The leakage he calculated, would correspond with a fall in the barometer of 1 inch per minute, when the action of the engine was suspended. Under these circumstances the velocity of the train would be as follows, when

7	miles from	the engine	2	miles	per hour.
4	,,	,,	17	,,	,,
3	,,	,,	25	,,	,,
2	,,	,,	35	,,	,,
1	,,	,,	55	,,	,,

providing the gradients could be so adapted as to balance the variable resistance occasioned by the variable velocity of the train through the external air, and the velocity of the engine could also be suitably varied, so as to extract the air at the proper velocities.

Neither of these conditions could be attained in practice; consequently the velocities thus marked could not be arrived at, except with trains of very light weight, and in fact, the whole motive force of 20½ inches of mercury would be expended, in impelling a train of 40 tons weight up an inclined plane of 1 in 100, with a velocity of no more than 30 miles per hour.

The mechanical difficulties would be materially relieved by employing tubes of larger diameter; for cæteris paribus, the velocit was always as $\sqrt{d}$; but then the engine power and the weight of the tube, and consequently the cost of construction, would thereby be greatly increased, and would probably, in an economical point of view, more than counterbalance the advantages of greater load and *higher velocity.*

Mr. Scott Russell had only a single statement to make on the subject of the cost of the Atmospheric system, which however had also some bearing on the scientific questions involving the soundness of Mr. Stephenson's experiments. It related to the resistance to be overcome by Railway trains drawn by the Atmospheric system. It had been alleged that Mr. Stephenson's observations did not truly represent the resistance of a train of carriages, for it was asserted, that the trains would have attained a much higher velocity with the force employed. The question to be determined, therefore, was the actual resistance of a train of carriages similar to that employed on the Dalkey Railway, at the speed there given. Mr. Russell had made accurate experiments on this subject, on trains under similar circumstances to those recorded in Mr. Stephenson's experiments, at velocities of from 30 miles to 33 miles an hour. Now he had compared these with two recorded in the Report, namely, the 5th and 7th experiments, in which the circumstances were very nearly similar. The coincidence was remarkable. He had found the total resistance of similar trains, at the velocities of the 5th and 7th experiments, to amount to 22½ lbs. per ton. He had also calculated the power exerted by the atmospheric pressure on the piston, as given in Mr. Stephenson's experiments on the pressure gauge of the piston, and had found it amount to the same sum, with an allowance only of a small quantity for the friction of the piston and valve apparatus, and the curvature of the line. This result was further confirmed by Mr. Bidder's analysis of Mr. Stephenson's experiments, where it was demonstrated, that in the 5th and 7th experiments the resistance shown by the Atmospheric gauge was from 21 lbs. to 23½ lbs. per ton. This was a close coincidence, if the difference of the method of making the experiments was taken into account.

Mr. C. May would have preferred the statement of facts rather than the multitude of mathematical propositions that had been indulged in; perhaps however considering the limited extent of the present application of the Atmospheric system, it was difficult to arrive at more facts than had been given. If in a discussion upon the performances of locomotive engines, any comparison between those of the present time and those of an early period of their introduction, should be made, the inference would be questioned; he contended, on the same grounds, that any comparison between the duty of locomotives in their present improved state and that of the Atmospheric system, so recently proposed, would be liable to the same objections, as it was only fair to presume that ameliorations would be introduced into the latter, as they had been into the former system. With respect to the *practical difficulties* that had been urged against the system, he attached but little importance to them, as so much had been done, as to warrant the assumption of the possibility of much more being accomplished.

Mr. N. **Wood** stated, that one of the earliest locomotive engines, made by Stephenson for the Darlington Railway, was now conveying coals at a less cost per ton than on any other railway in England.

Mr. **Bidder** contended, that the comparison of the two systems in their present state was not so fallacious as had been asserted. The propelling power of the atmosphere upon the piston must always remain constant, and as regarded the formation of the vacuum, no machine had been more improved than the stationary steam engine, whether viewed with respect to the development of power or the cost of working. The great improvement therefore must be made in the continuous valve and the tube. In the former, it was stated, that the leakage was already reduced to an amount that had not been anticipated, and in the latter, the friction of the air would, he contended, be felt much more severely when the lengths were extended, than was at present allowed. As a practical comparison of the acquisition of speed by the two systems, he would instance the North Shields Railway which was 7 miles long, and upon which there occurred five stoppages in each journey, yet the distance was accomplished regularly in 20 minutes. Now to do this, the maximum speed of 30 miles per hour must necessarily be attained, within the distance of a mile from the point of stoppage.

Mr. R. **Nicholson** could not accord with Mr. Samuda's opinion, that a distance of 1¾ miles was requisite, to enable a locomotive engine, with a train of 30 tons weight, to acquire a velocity of 25 miles per hour. He thought the position was assumed on false data. The Newcastle and North Shields Railway, of which he was the engineer, was 7 miles in length; that distance had been regularly accomplished in 21 minutes by nearly fifty trains per day, for the last 5 years, calling at five stations on each journey, thus stopping and starting six times. Assuming the time occupied in stopping at each station to be ¾ minute, the actual time for running the entire distance was 17¼ minutes. This would give an average speed of 24 miles per hour, and showed a maximum velocity of upwards of 30 miles per hour, whilst the distance between the places of stopping was 1⅙ mile. The gradients were between 20 feet and 30 feet per mile, and the trains weighed from 23 tons to 25 tons.

Mr. Nicholson found, in some experiments made by him on the Newcastle and Carlisle Railway and on the Dalkey Line, in order to ascertain the distance in which a train could be stopped with and *without a locomotive* engine, that on the former railway, a train weighing 28 tons, travelling at the rate of 30 miles per hour upon a level plane, could, when attached to and assisted by a locomotive engine, be stopped in the space of 226 yards. The engine itself, when detached from the train, could, under similar circumstances, be brought to a stand still in about 100 yards. The train without the engine ran upwards of 400 yards. There were two breaks applied to the carriages and one to the tender, and the engine was reversed after a speed of about 15 miles per hour had been attained.

On the Dalkey line on a curve of a radius of 233 yards, rising at an inclination of 1 in 105, a train weighing 19¼ tons and travelling at the speed of 33¾ miles per hour, required a distance of 247 yards to stop it, with three breaks applied.

The weather was calm and the rails were in a favourable condition, when these experiments were made. The result of these experiments fully satisfied Mr. Nicholson that the locomotive engine was of considerable assistance in stopping a train, and that an engine and train, upon a locomotive line, could be stopped in a shorter distance than a train upon an atmospheric line, all the other circumstances being alike.

Mr. **Barlow** said, that a misunderstanding appeared to exist as to the novelty of the Atmospheric Railway, and as to the field it offered for improvement; it was in fact simply a revival of the old Stationary System. Mr. Samuda had not shown that there was less power lost in the Atmospheric than in the Stationary System, whereas Mr. Stephenson had demonstrated by his experiments, that there was more power lost, than by the system of rope traction. If the present novelty had been the rope instead of the atmospheric tube, precisely the same arguments would have been used in favour of it, as to avoiding the weight and danger of the locomotive, risk of collision, wear and weight of rails, &c., and the same disadvantages as well as advantages of the old stationary system would, he thought, be found to exist in the mode now proposed of its application by the atmospheric tube.

Mr. **Hawkshaw** thought the arguments which had been used to illustrate the commercial view of the question, although simple were not at all conclusive. It appeared, that the intention was to demonstrate, that the interest alone of the first cost of the Atmospheric Railway would be equal to the actual cost of working the line by locomotive power; but in taking this view, an important element had, he submitted, been neglected. He would assume that which he believed was generally admitted, that the chief wear and tear of the rails, &c., was due to the great weight of the locomotive engines. If they could be dispensed with, the rails would last much longer. The value of the item of destruction of rails could be shown, by assuming that they would be worn out in ten years; allowing 3 cwt. or 30 shillings' worth of iron per yard, an expenditure of £264 per annum would be incurred for rails alone, upon a line of a given length.

The question of adopting the Atmospheric system did not, however, depend upon the difference of a few shillings of expense either way; the real questions were the safety and the comfort to the passengers, which the public could more readily appreciate. It appeared evident to him, from what he had seen on the Dalkey line, that although much more experience was required in carrying out the system, there were many situations where it could be very advantageously adopted, in preference to locomotive engines, even if it were more expensive, which however was not, in his opinion, as yet satisfactorily proved.

Mr. Hawkshaw regretted to hear, what amounted to an assertion, that no improvement had been made in the locomotive engine during the last 12 years. His experience led him to a very different conclusion, and he thought he should be supported in the opinion that engines generally, were now doing the same amount of work at an infinitely less rate of cost than they did 12 years ago.

Mr. Samuda regretted his unavoidable absence from some part of the previous discussion, as there were many interesting points alluded to which he could have wished to have replied to, but which must now pass unnoticed.

In reference to Mr. Hawksley's formula, Mr. Samuda could not pretend to examine into its correctness before the meeting, as it evidently required much careful study to appreciate its bearing upon the atmospheric system. The result of the working on the Dalkey Railway had shown practically how inconsiderable was the difference of pressure between the two extremities of a column of air 1¾ mile in length, and he was satisfied that in practice that difference might be altogether disregarded.

The experiments made by Mr. Samuda and published in the parliamentary evidence had been referred to for controverting this point; but a proper examination of those experiments would, he contended, entirely support the conclusions he had arrived at; for the variation between the barometers—as a general rule—it would be observed, was there constant,—whether near the engine, or far from it,—whether travelling 40 miles an hour, or at rest—in all cases from about 1 inch to 1½ inch was the utmost observed difference;—the fact being, that the difference was mainly caused by the proportion the leakage bore to the receiver it was leaking into, and was very slightly influenced by any other cause. In bringing this discussion to a close Mr. Samuda was content to leave to facts—not words—the settlement of the wide